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Substitution of another opioid for morphine. Opioid toxicity should be managed initially by decreasing the opioid dose

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Hypertension treatment and control in sub-Saharan Africa

Figure of \$1800 per life saved seems optimistic

EDITOR—Cooper et al call for the inclusion of antihypertensive treatment among health priorities in sub-Saharan Africa.¹ Assuming that the patient needs one drug to control his or her blood pressure and that compliance is good, they estimate that one death could be prevented at a cost of \$1800 (£1130).

Some patients need two or more drugs to control their hypertension, and compliance rates for chronic diseases in Africa are low for many reasons, including poverty, lack of access to health services, mobility of the population, and a lack of understanding of the nature of chronic disease. In the Republic of Congo patients arrive at a health centre to be cured. A doctor who can only control an illness is often seen as having failed, and the patient will then either try a new doctor or consult the local healer.

The Centre Médical Evangelique runs an externally funded project for patients with epilepsy. The cost of the treatment is only \$18-\$24 (£11.70-£15.10) a year, and most patients see a considerable change in their condition. The programme is mobile to facilitate access. Despite this the average non-attendance at any consultation is about

40%. This figure is likely to be higher for a similar programme of antihypertensive treatment, because the patient often experiences no improvement in symptoms and may have side effects from the medicine prescribed.

I think that Cooper et al's figure of \$1800 per life saved is optimistic. Even if accurate it means that antihypertensive treatment cannot rank alongside the provision of clean water, vaccination, and oral rehydration in terms of lives saved per unit cost. So what should be done for hypertension in sub-Saharan Africa? I believe we should treat patients with symptoms or complications, or both, as they are more likely to be compliant with treatment. We should also treat those who want to be treated and who understand the importance of continuing treatment. Doctors and other interested parties need to disseminate information to their communities about the nature and risks of hypertension.

Finally, the authors do not mention the value of dietary salt restriction. This is cheap and particularly effective in African populations.² Measures such as this would have a greater impact on the prevalence of hypertension in the African continent.

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1 Cooper RS, Rotimi CN, Kaufman JS, Muna WFT, Mensah GA. Hypertension treatment and control in sub-Saharan Africa: the epidemiological basis for policy. *BMJ* 1998; 316:614-7. (21 February.)

2 *La lutte contre l'hypertension. Rapport d'un comité OMS d'experts*. Geneva: Organisation Mondiale de la Santé, 1996. (OMS; Série de rapports techniques No 862.)

Amount spent on health care per capita is same as cost of a McDonald's

EDITOR—Cooper et al's article on hypertension treatment and control in sub-Saharan Africa was another example of cultural (medical) imperialism.¹ The science is sound and the argument reasonable, but the article shows how out of touch academics in learned institutions are with medicine at the grass roots.

I ran a hospital in rural Uganda for some years. The way in which nearly all health care is provided in Uganda and neighbouring countries is commercial: health care has to be paid for. People delivering it have to receive their income from the patients they treat. A patient attending a hospital or clinic has to pay a fee for consultation, fees for investigations, and

then the cost of the drugs. The institutions depend exclusively on this income to sustain themselves. Even a programme supplying free antihypertensive drugs to clinics would not get round that problem.

The average amount of money spent per head of population each year is about the cost of a meal at McDonald's, and medical care is sought only in an emergency. How anyone in the West could expect a peasant farmer to spend £22 a year on a drug that treats a disease that causes him or her no suffering is beyond me. It is absurd to suggest that over £1000 a year should be spent to save one life from hypertension when the great majority of these people don't have clean water to drink. The hospital I worked in saved hundreds, if not thousands, of lives, each one at a cost of a fraction of £1000. I struggled desperately to make ends meet and keep it going. A mere £30 000 makes the difference between collapse and survival.

Virtually the only section of the article that prompts my agreement is the very last clause: "instead ... investment in an organised care system would reap large gains in adult health."

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1 Cooper RS, Rotimi CN, Kaufman JS, Muna WFT, Mensah GA. Hypertension treatment and control in sub-Saharan Africa: the epidemiological basis for policy. *BMJ* 1998; 316:614-7. (21 February.)

Burden of cerebrovascular disease will increase as more people survive to old age

EDITOR—We agree with Cooper et al's conclusion that hypertension is worth treating in sub-Saharan Africa but would like to raise two points.¹ Although in developed countries more deaths from ischaemic heart disease than stroke are attributable to hypertension,² this is not true of sub-Saharan Africa, where (particularly in rural areas) ischaemic heart disease is rare; thus care needs to be taken when one is comparing figures in which cardiovascular diseases are grouped together. The limited data on stroke that exist for sub-Saharan Africa mainly relate to hospital studies. They suggest high rates of hypertension, and high rates of default from treatment for hypertension, in those patients who attend hospital with stroke.³

While knowledge of cause specific mortality in adults in sub-Saharan Africa is limited, so also is information about cause specific morbidity. Cerebrovascular disease is a major cause of disability in people who survive stroke in developed countries.

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Although absolute numbers in sub-Saharan Africa are currently low,⁴ as the proportion of people surviving to old age increases so will the burden of cerebrovascular disease; age standardised mortality already exceeds that in the United Kingdom.⁵ The most important risk factor for stroke amenable to modification is hypertension, and we are piloting cost effective methods of controlling hypertension in rural and urban areas through the "health in the next millennium" programme of the Department for International Development.

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- 1 Cooper RS, Rotimi CN, Kaufman JS, Muna WFT, Mensah GA. Hypertension treatment and control in sub-Saharan Africa: the epidemiological basis for policy. *BMJ* 1998;316:614-7. (21 February).
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Authors' reply

EDITOR—We agree with Burdon that our estimate of efficacy may not hold for all communities. Our interest was to advance a discussion based on evidence. For example, while it is true that some patients will require two or more drugs, it would be useful to undertake studies to determine how much risk reduction would occur with a simple regimen of one drug versus tailored multi-drug treatment. We also need to evaluate the proportion of patients who will continue to take treatment long term; this may vary widely from one setting to the next. In the United States, after 30 years of effort, hypertension is controlled in only a quarter of hypertensive patients; we should not abandon interventions in Africa because they fail to meet an unreasonable standard.

We understand Burdon's motivation to restrict treatment to patients who have end organ damage or symptoms, but we believe that this is too late—renal failure or stroke is often a death sentence in Africa. Furthermore, it concentrates the effort on such a small group of patients that the investment in the whole process has little payoff. We cannot find any evidence for concluding that compliance among asymptomatic hypertensive patients is an overwhelming obstacle. In our communities in Nigeria and Cameroon a vigorous campaign to increase awareness has resulted in many patients taking their pills faithfully, although we cannot quantify that effect. We are wary of authoritative proclamations that are based on opinion alone. Non-pharmacological interventions

have considerable appeal. Unfortunately, they do not work for patients with severe hypertension.

We are no more sympathetic to Montgomery's position than he is to ours. We may sound like medical imperialists, but he sounds like Albert Schweitzer. Presumably he believes that Africans should not be treated for hypertension; at the very least he is out of touch—millions are. In Cameroon, for example, government programmes exist to provide care for hypertension and diabetes. And why not? Because the patients also need clean water? If they need treatment, and are being treated, should we not have a policy? Perhaps antihypertensive treatment will be irrational in some African settings; it may also be highly appropriate in others. It is better to answer those questions with data.

We appreciate the comments from Walker et al. The group in Tanzania is making enormous contributions to our understanding of chronic disease in Africa.

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Ion channels

Ion specificity of motor end plate acetylcholine receptor

EDITOR—I was surprised by the assertion of Laniado et al that "acetylcholine...acts at the postsynaptic membrane of the motor end plate to open chloride channels."¹ In a series of classic experiments in the 1960s, Takeuchi and Takeuchi used glass micro-electrodes to clamp the end plate region of partially curarised frog sartorius muscle.² The end plate current decreased as the membrane potential was clamped at increasing values from its starting value of -100 mV. End plate current was zero at a membrane potential of -15 mV (the reversal potential). This reversal potential was affected by changing external sodium and potassium concentration (becoming more positive with increasing cation concentrations), but it was not affected by changing the chloride concentration. This result suggested that the channel was permeable to sodium and potassium but not to chloride ions. Further studies extended these conclusions, and the channel was found to be permeable to calcium and magnesium and organic cations (but not anions). Indeed, the finding that the channel permeability for divalent cations is somewhat less at high cation concentrations has led to the suggestion that there are negative charges on the external surface near the mouth of the channel.³

Other studies analysing the reversal potential have shown that chloride channels

are associated with γ -aminobutyric acid receptors⁴ and glycine activated channels.⁵ I am not, however, aware of studies showing that the action of acetylcholine at the motor end plate opens chloride channels, either in mammals or in other species.

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- 1 Laniado ME, Abel PD, Lalani E-N. Ion channels. *BMJ* 1997;315:1171-2. (8 November.)
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Authors' reply

EDITOR—Pandit has drawn attention to the action of acetylcholine and ion channels in relation to the motor end plate. Acetylcholine can result in the opening of chloride channels in other locations, and we acknowledge him for providing a succinct explanation of their relation.^{1 2}

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- 1 Ikeda K, Wu D, Takasaka T. Inhibition of acetylcholine-evoked Cl^- currents by 14-membered macrolide antibiotics in isolated acinar cells of the guinea pig nasal gland. *Am J Respir Cell Mol Biol* 1995;13:449-54.
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Mistaken subdural cannulation can produce Horner's syndrome

EDITOR—Minerva showed a photograph of a patient with Horner's syndrome after a supposed epidural infusion.¹ I have seen a similar presentation after insertion of what I had presumed to be an epidural catheter. However, with the development of unilateral Horner's syndrome and a high patchy block with sacral sparing it became apparent that the catheter was in fact in the subdural space. Subdural cannulation is said to occur in 0.1-0.8% of all epidurals and is often missed.²

I suspect that this is what happened to Paw's patient. The risk of rupture of the delicate arachnoid matter resulting in total spinal anaesthesia makes it mandatory to remove and resite the catheter in the epidural space.

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- 1 Paw H. Minerva. *BMJ* 1998;316:160. (10 January.)
- 2 Datta S. *The obstetric anaesthesia handbook* 1995. 2nd ed. St Louis: Mosby, 1995:134.

Incorporating patient preferences into clinical trials

Information about patients' preference must be obtained first

EDITOR—Torgerson and Sibbald discuss the difficulties of assessing the relative merits of treatments when patients have strong preferences for one of the alternatives.¹ In these circumstances, however, patients should not be expected to participate in randomised comparisons, and neither should the professionals caring for them.

It is important to consider the bases of these preferences, particularly as there is a widespread and unsupported belief that new treatments are likely to be superior to existing alternatives.² For example, it seems that people with diabetes who were being recruited to a randomised comparison of insulin pumps with conventional management were left with the impression that pumps represented an important advance (C Bradley, personal communication). Not surprisingly, therefore, those allocated to pumps were pleased, while those allocated to conventional management were disappointed. Randomisation thus created comparison groups that were incomparable in these psychological characteristics, and this may have had implications for compliance and evaluation of treatment outcome.³

Bradley's response³ was to propose the partially randomised patient preference design to which Torgerson and Sibbald refer. Unfortunately, this does not help because it cannot distinguish between an effect of preferences and an effect of confounding of preferences with prognosis. Since it is impossible to randomise between sincerely held preferences, measuring their effects reliably requires a more complicated design, which was suggested originally by Rucker⁴ and recently discussed by McPherson et al.⁵ In this design, people are randomised between either a randomised comparison or a preference comparison.

Genuine therapeutic effects may, however, be associated with preferences, over and above those related to adherence to treatment. Several blind trials show an advantage associated with adherence to placebo.⁵ Obtaining hard evidence on possible preference effects is problematic as it is difficult to distinguish reliably between simple therapeutic effects and preference effects mediated through psychological pathways in experiments.

There are thus two areas that need attention. Firstly, there needs to be wider acknowledgement that preferences for treatments should be based on beliefs that are founded on reliable information. This should help to increase the proportion of well informed people who have no strong preferences and would thus be eligible to participate in comparisons between randomised treatments. Secondly, studies are required to enable a rigorous distinction to be made between simple therapeutic effects and preference effects. This means that well

accepted biological hypotheses will be needed for adequate recruitment, and a plausible biological model to distinguish the two kinds of effect. As Torgerson and Sibbald suggest, however, the first step is routinely to elicit information about the preferences of well informed patients.

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- 1 Torgerson DJ, Sibbald B. What is a patient preference trial? *BMJ* 1998;316:360. (31 January.)
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Merits of alternative strategies for incorporating patient preferences into clinical trials must be considered carefully

EDITOR—In their overview of patient preference trials, Torgerson and Sibbald suggest that, given the potential drawbacks of such designs, research might usefully take the alternative approach of measuring patient preferences within a traditional randomised controlled trials design.¹ This would conserve "the advantages of a fully randomised design with the additional benefit of allowing for the interaction between preference and outcome to be assessed." We used Torgerson and Sibbald's approach to compare two schedules of routine antenatal visits.² Our unpublished findings on patient preferences show how such analysis can extend and clarify trial findings.

We stratified our two groups—new style care (6-7 antenatal visits) and traditional care (13 antenatal visits)—by the initial preferences of the women who took part. Within each stratum we compared those allocated to traditional and new style care for one key acceptability outcome (dissatisfaction with the frequency of antenatal visits) and one key outcome relating to psychosocial effectiveness (negative attitude to the fetus). The findings are shown in the table.

Comparison of two groups of women for one key acceptability outcome (dissatisfaction with frequency of antenatal visits) and one key outcome relating to psychosocial effectiveness (negative attitude to fetus)

Initial preference	Allocated to traditional care	Allocated to new style care	Odds ratio (95% CI)*
Dissatisfaction with frequency of visits (%)			
None	7.4 (29/391)	31.0 (135/435)	5.62 (3.61 to 8.94)
Traditional care	3.5 (11/317)	71.1 (118/166)	68.39 (33.31 to 149.14)
New style care	48.9 (110/225)	12.2 (36/294)	0.15 (0.01 to 0.23)
Negative attitude to fetus (mean (SD))			
None	6.0 (4.25)	6.8 (4.26)	0.003
Traditional care	6.3 (4.05)	7.5 (3.83)	0.005
New style care	5.7 (4.12)	5.8 (3.77)	0.452

*P value for negative attitude to fetus.

Our main overall finding of greater dissatisfaction in the new style group applies only to those who had either an initial preference for traditional care or no initial preference. When women had an initial preference for new style care the effect was in the opposite direction. Similarly, the finding that women in the new style group had a more negative attitude to their fetuses only applies to those who initially preferred traditional care or had no initial preference. This analysis provides information that is relevant for new policies: women with an active preference for fewer visits should not be denied this option because of concern about possible detrimental psychosocial effects.

A partially randomised patient preference design would have yielded similar findings, but at the cost of a substantial increase in sample size. We assumed that all those with an initial preference would have opted for their preferred type of care and that eligible women who declined participation because they did not want to have fewer antenatal visits would have taken part if it had been a preference trial. We calculate that the required overall sample size would have increased from 2830 to 7989. This highlights the need to consider carefully the respective merits of alternative strategies for incorporating patient preferences into clinical trials.

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- 1 Torgerson DJ, Sibbald B. What is a patient preference trial? *BMJ* 1998;316:360. (31 January.)
- 2 Sikorski J, Wilson J, Clement S, Das S, Smeeton N. A randomised controlled trial comparing two schedules of antenatal visits: the antenatal care project. *BMJ* 1996; 312:546-53.

Authors' reply

EDITOR—McPherson and Chalmers are correct that more research is required into the interaction between preferences and outcome. Indeed, the first randomised trial comparing an ordinary randomised trial and a patient preference trial has just been published.¹ This trial showed no difference in recruitment and retention rates between the two randomised segments of the trials.

Clearly it is important that patients are given reliable information. It is, however, likely that some patients have preferences and are still prepared to be randomised. What should be done with such patients? Including them in a "partially randomised preference trial" is unsatisfactory, as McPherson and Chalmers state. Randomising patients between a fully randomised design and a preference trial is problematic, as suggested, not least because those patients allocated to a fully randomised design must be denied their preferences if full assessment of preferences is to be taken into account. This is probably ethical if the preferred treatment is available only to randomised patients.

Randomising all consenting patients and eliciting their treatment preferences may help.² Clement et al show that this is both feasible and a better alternative to the partially randomised design. They are, however, incorrect to assume that a partially randomised patient preference trial would have yielded similar findings. The preference arms in a preference trial could have been subject to confounding and, furthermore, would not have yielded information on the dissatisfaction rates and attitude scores of women allocated to their unpreferred treatment.

The importance of this is illustrated in the findings of their previous paper. The odds ratio of dissatisfaction was 2.50 (95% confidence interval 2.00 to 3.11) for the new group compared with the traditional group.³ When all women with an initial preference are removed (hence achieving a balance of preference between the two groups) the odds ratio increases to 5.62 (3.61 to 8.94), thus indicating a significant effect of preference on this trial outcome.

McPherson et al point out the sample size required to test for any interaction between preference and outcome would need to be relatively large.⁴ Many trials may be too small to examine the interaction between preference and outcome. One solution may be to try to standardise how preferences are elicited and then use meta-analytical techniques by combining a number of trials to assess differences in outcome by preference.

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Potential biases do not affect results of waiting time study

EDITOR—Nick Black's helpful letter¹ raises four potential biases in my study that may affect the conclusion that fundholding reduces waiting times.²

His first point concerned the accuracy of my database. I have now made random checks of patients' details at general practices in the catchment areas of all four providers. These confirmed the accuracy of the database.

The second was that if the proportion of patients of fundholders and non-fundholders placed on the elective waiting list differed this could cause contrasts in waiting times. Overall, 61% of non-fundholders' patients and nearly 60.6% of fundholders' were placed on the elective waiting list—a marginal difference. Anyway, individual patients of fundholders still had shorter waits regardless of whether there was proportionally more or less of them than patients of non-fundholders.

The significance of case mix variation is best measured by the final mix of procedures rather than by an age, sex, or diagnosis profile. This is because long waits for specific types of operations may affect waiting time differences if the incidence of those operations is far greater in one population than the other. Analysis by the χ^2 test for independence shows the mix of procedures to be broadly similar, although in 11 of the 16 cases significant differences occurred between actual and expected activity for a few (at most three) of the 10 most common procedures. In five of these 11, case mix actually increased the waits of fundholders more than non-fundholders. In the other six, increases to the waits of non-fundholders, relative to fundholders, were too marginal to reverse the original results.

Median waits of patients of non-fundholders and fundholders for elective operations covered by fundholding scheme for four NHS providers over four years

Provider and year	Non-fundholders (days)	Fundholders (days)
Crawley and Horsham:		
1992-3	69	62
1993-4	167	65
1994-5	132	58
1995-6	168	115
Mid-Sussex:		
1992-3	54	44
1993-4	62	43
1994-5	62	44.5
1995-6	59	47
Royal West Sussex:		
1992-3	176	61
1993-4	245	105
1994-5	284	126
1995-6	153	65
Worthing and Southlands:		
1992-3	74	48
1993-4	83	59
1994-5	80	62
1995-6	83	83

The fourth doubt concerned the use of the mean as a measure of waiting time rather than the median. However, the analysis of variance tests used to compute the significance of differences do take account of variances within columns (within the same population) as well as between them. The table shows the median waits. These confirm the results shown by the mean with the exception of Worthing and Southlands in 1995-6. In the other 15 cases patients of fundholders still have appreciably shorter waits. Therefore, my original conclusion that fundholding reduces waiting times seems justified.

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- Black N. Potential biases were not taken into account in study of waiting times. *BMJ* 1998;316:149. (10 January.)
- Dowling B. Effect of fundholding on waiting times: database study. *BMJ* 1997;315:290-2. (2 August.)

Temporary pacing before permanent pacing should be avoided unless essential

EDITOR—Fitchet and Fitzpatrick report a case of air embolism resulting from a poorly sealed temporary pacing wire,¹ but they do not discuss an important point—namely, that insertion of a temporary pacing wire before permanent pacing is hazardous and should be avoided unless essential.² Insertion of a temporary wire in such circumstances combines the risks of central venous access with those of systemic infection while fouling one potential vascular access site.

At Papworth Hospital 15% of patients admitted for permanent pacing are transferred from another hospital with a temporary pacing wire in situ.³ A considerable proportion of these patients develop systemic infection as a result, requiring intravenous antibiotics; permanent pacing then has to be delayed by up to a week (of expensive in-hospital stay).³ Irrespective of this, patients with a temporary wire have a sixfold increased risk of infection of their permanent pacing system.⁴ This outcome can be associated with a mortality of up to 50% if infected leads are not removed⁵—a procedure that itself has a 2% mortality.

The 71 year old woman in the case reported had bifascicular block and asymptomatic intermittent complete heart block. This arrhythmia does not require temporary pacing, and recognition of this fact would have prevented the unfortunate consequences. Patients are frequently transferred from district hospitals to specialist centres with unnecessary temporary pacing wires.³ We suggest that, if there is doubt, the decision to insert a temporary system before permanent pacing should be made in conjunction with the specialist centre.

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- 1 Fitchet A, Fitzpatrick AP. Central venous air embolism causing pulmonary oedema mimicking left ventricular failure. *BMJ* 1998;316:604-6. (21 February.)
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People at risk of coronary heart disease should not be denied treatment with effective drugs for purely financial reasons

EDITOR—As soon as effective treatments for coronary heart disease—which causes 30% of deaths in the United Kingdom—became available, evidence based medicine and finance clashed. The Standing Medical Advisory Committee attempted to impose the Sheffield risk tables¹ and was condemned for its simplistic guidelines and high threshold for treatment.² A report on cholesterol and heart disease by the NHS Centre for Reviews and Dissemination now seeks to reduce expenditure on statins by focusing clinical effort on modification of lifestyle and treatment for mild hypertension.³

The relation between cholesterol and coronary heart disease is log-linear so a “normal” cholesterol concentration is a fallacy. Most people in the United Kingdom have high cholesterol concentrations and would benefit from lipid lowering. To reduce coronary heart disease in the United Kingdom people must change their lifestyles, but evidence for the success of lifestyle modification is depressing: participants in the OXCHECK study reduced their cholesterol concentrations by only 3% and their body mass by 2%.⁴

The report admits that lowering cholesterol concentrations is beneficial but claims that statins are too expensive, compared with treatment for hypertension with off patent drugs. Many patients with hypertension, however, receive multiple patented drugs at a cost similar to that of statins.

The cost of treatment of hypertension—number needed to treat=40 at a blood pressure of 170/100 mm Hg—is similar to that of primary prevention for high risk patients with cholesterol concentrations of 7 mmol/l. The west of Scotland coronary prevention study showed a 30% reduction in mortality despite a 30% dropout rate.⁵ The 7-year extension of the Scandinavian simvastatin survival study suggests a reduction of 47% in event rates in the treatment group. Lipid lowering may therefore be even more cost effective than previously believed.

Treatment with statins is more effective than multidrug treatment for hypertension in reducing absolute risks: a 30% reduction can be achieved with a single drug, compared with 10% reductions for any single antihyper-

tensive drug. Statins also have fewer side effects. The issue of statins approaching expiry of their patent is irrelevant. It is unethical not to treat people at risk of coronary heart disease with effective drugs for purely financial reasons—just as it would be unethical not to treat those who smoke or are obese.

The report leaves us little further forward in deciding whom we should treat or where the necessary money should come from. Unfortunately its press release and the subsequent media reports convey a negative message to healthcare providers and to those patients who need to have their lipids measured and adequately treated.

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Dilemmas exist in withdrawing ventilation from dying children

EDITOR—We agree with Jonathan Gillis about the guidelines of the Royal College of Paediatrics and Child Health on withholding or withdrawing life saving treatment in children.^{1 2} There is little doubt about the need for such a document. Modern treatment has made it possible to prolong life beyond the point where continued treatment is physiologically futile and the benefits of living might be outweighed by the physical and psychological cost of intensive care. The document reinforces the stand point that there should be no ethical difference between withholding and withdrawing treatment, but Gillis points out that in practice there is an emotional difference. His view is supported by our recent survey of 73 consultants in paediatric intensive care in the United Kingdom which documented the uncertainty surrounding these issues.³ Fifty one per cent of respondents believed there to be a moral difference between withholding and withdrawing ventilatory support, contrary to current published opinion.⁴

Our survey focused primarily on how withdrawal was accomplished; 42% of respondents preferred extubation to terminal weaning of mechanical ventilation, with nearly half (49%) using a higher dose of sedative during withdrawal. Although the guidelines allow the use of sedative drugs in increased dose during this process, the

primary intention should be to relieve discomfort rather than hasten death. Nine respondents (12%) continued paralytic agents during extubation, presumably to abolish the distress of agonal respiratory efforts, but undeniably speeding the process of death. The college document supports the continuation of paralysis under these circumstances, yet some physicians would consider this practice tantamount to euthanasia.⁵

In our own practice we recognise that withdrawal should proceed swiftly with the minimum distress to the child and family and that under these circumstances it may be appropriate to extubate dying children under high dose analgesia and sedation. However, it is not current policy in our unit to continue neuromuscular paralysis during the process of withdrawal.

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Prescribing patient information leaflets may be better than prescribing drugs

EDITOR—Greenhalgh and Gill highlight the “disparate factors on the decision to prescribe,” but they offer little advice in terms of strategies to resist the pressure to prescribe.¹ They describe a study in which 22% of prescriptions were thought to be not strictly indicated by the prescribing general practitioner.² Giving an information leaflet on some of these occasions (for example, for self limiting illnesses) may be an alternative to a prescription and often more appropriate. In addition, giving written information reduces re-consultation rates for some recurring conditions, which may also reduce prescribing.^{3 4} This is apart from the many other benefits to patients who receive written information to complement the verbal advice from their doctor.⁵

To “prescribe” a leaflet for most common conditions seen in general practice is now routinely possible. Comprehensive databases of patient leaflets are now a standard part of general practice computing. A leaflet can be accessed quickly and can be customised, personalised, and printed as part of the consultation. Patient leaflets are also being linked to clinical decision support systems. For example, when accessing one of the 240 clinical guidelines in PRODIGY (a computerised prescribing decision support system for general practitioners in the

United Kingdom) the general practitioner is offered the option of printing a leaflet to give to the patient about his or her illness in addition to any prescription options.

Printing and issuing a personalised patient leaflet can be almost as quick as printing a prescription. Further studies are required to show the acceptability of this alternative and in which situations their use is most appropriate. Even a small shift towards issuing a "PIL" (patient information leaflet) instead of a prescription for pills will result in considerable savings to the drug budget.

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Substitution of another opioid for morphine

Opioid toxicity should be managed initially by decreasing the opioid dose

EDITOR—We would like to clarify some important points arising from Murray's letter about the substitution of another opioid for morphine being of use in pain control.¹ The first is the description of pain being "relatively resistant to morphine." It is more useful to think of responsiveness to opioids as a continuum; the factors that generally decide the position on the continuum are the side effects of opioids, particularly sedation.² Some pains, especially neuropathic pain, require larger doses of opioids, which consequently give rise to more troublesome side effects and thus limit an escalation of the dose and the achievement of adequate analgesia. Opioid toxicity results from an unfavourable balance between analgesia and the side effects of opioids.

While alternative opioids may produce a better outcome,³ the safest and most efficient management of opioid toxicity is to reduce the opioid dose in the first instance rather than switch to an alternative opioid. This approach commonly achieves the desired balance between analgesia and side effects.⁴

Murray's comment about ketamine (an N-methyl D-aspartate (NMDA) antagonist) in neuropathic pain is interesting. The NMDA antagonists are promising drugs for the treatment of pain syndromes with the clinical phenomena of central "wind-up"

(allodynia, hyperalgesia, hyperpathia). The opioid mechanism does, however, seem to be crucial here also; NMDA antagonists seem to renew the opioid responsiveness in vitro and in the clinical situation.⁵

While alternative opioids to morphine, and the NMDA antagonists, are important as analgesics, we would reinforce a simple, systematic approach to pain control as outlined in the first paper in the ABC series on palliative care. In particular, opioid toxicity should be managed initially by decreasing the opioid dose rather than automatically switching to an alternative opioid.

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Methadone can be used to manage neuropathic pain related to cancer

EDITOR—Murray fails to mention the potential that methadone has for managing neuropathic pain related to cancer, advocating the use of ketamine.¹ Evidence is increasing that hypersensitisation states characteristic of neuropathic pain involve activation of the N-methyl D-aspartate (NMDA) receptor.² Activation of this receptor has a crucial role in the development of tolerance to the analgesic effects of morphine.

Treating such patients with drugs with NMDA receptor antagonist properties may attenuate this tolerance. The affinity of methadone for the NMDA receptor is similar to that of ketamine³; furthermore, like morphine and unlike ketamine, methadone has a high affinity for the μ receptor, where it acts as a full agonist. There are thus theoretical advantages in using a μ opioid agonist that also acts as a non-competitive NMDA receptor antagonist—that is, methadone. This has led us to apply revised guidelines for the use of methadone in cancer pain with a dominant neuropathic component.³

The renewed interest in methadone and the emergence of new strong opioids (fentanyl, hydromorphone, and oxycodone) on the United Kingdom market may lead to confusion among health professionals and detract from the optimum use of morphine in the management of chronic cancer pain. Indications for the use of alternatives to morphine require clarification.

In our opinion, there are two important indications for choosing alternatives to morphine. One of these is when, during continuous use, intolerable central nervous system side effects develop and reducing the dose has no effect or leads to increased pain. The second indication is when dose limiting side effects of morphine occur, prohibiting

an increase in dose and giving rise to inadequate analgesia (despite the use of coanalgesics and techniques appropriate to the pain syndrome).

The reason why opioid substitution is successful in these cases remains unclear. The development of severe central nervous system side effects has been associated with the accumulation of large amounts of the morphine metabolite morphine-3-glucuronide. This has central stimulatory properties with a potency several hundred times that of morphine,⁴ and thus a reduction of side effects after the substitution of an opioid without known active metabolites (for example, fentanyl or methadone) is explained. In some patients, pain that is poorly responsive to morphine may arise because of the development of tolerance to morphine. If tolerance to side effects does not develop to the same extent as tolerance to analgesia the escalating dose of morphine may reach a level at which the side effects become dominant.⁵

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Consideration of short term consequences of heavier babies is important

EDITOR—I read with interest the paper by Bonellie and Raab about the increase in birth weight among babies born in Scotland between 1980 and 1992.¹ Various factors were proposed as being responsible for this increase including a reduction in the proportion of induced births, lowered rates of cigarette smoking, and improvements in maternal diet and lifestyle.

During the same period we conducted a prospective study on the predictability of shoulder dystocia, an obstetric complication that is potentially catastrophic for both mother and baby, at the National Maternity Hospital in Dublin.² Our results confirmed existing data that the incidence of shoulder dystocia increases as birth weight increases. The overall incidence of shoulder dystocia was 0.6% (66/10 468) but the incidence among babies weighing from 4 to 4.5 kg was 1.9% (35/1790), and among those weighing more than 4.5 kg was 5.8% (23/395). While it was difficult to accurately compare the incidence of shoulder dystocia in 1992 with that from a decade or so earlier, our impression was that the

incidence was increasing due to an increase in the number of heavier babies.

We therefore compared the incidence of babies weighing >4 kg and of babies weighing >4.5 kg in 1979 and 1992. The incidence of babies weighing >4 kg in 1979 was 12.5% (1143/9159) compared with 19.4% (1220/6293) in 1992, and of babies weighing >4.5 kg was 2.9% (269/9159) in 1979 and 4% (252/6293) in 1992. This occurred during a time when rates of cigarette smoking and induction of labour were essentially similar.

During labour heavier babies are associated with a higher incidence of dystocia.³ Apart from the problem of shoulder dystocia, this trend towards a greater number of heavier babies will undoubtedly lead to increased obstetric intervention. There has been a general increase in the rates of caesarean section both in the United Kingdom and Ireland in recent years. While the reasons for this are multifactorial, the increasing birth weight and the greater number of heavier babies are certain to be important factors. While it is interesting to consider the long term implications of rising birth weight on the pattern of adult disease,⁴ we should first consider the problems that may arise in the shorter term.

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Genetic counselling must be non-directive

EDITOR—Minerva seems to hold the view that the aim of genetic counselling for parents who have a child with cystic fibrosis is to prevent them having further affected children.¹ She reports that of 42 families, 24 had declined prenatal testing and eight had accepted prenatal diagnosis but had decided against termination. She concludes that "part of the problem seems to have been poor communication between the doctors who should be providing the advice."

I had always been under the impression that counselling should be non-directive. Minerva perhaps does not understand that many parents do not want to undergo tests that might lead on to the abortion of their child; or even that many parents, having had a positive test result, still feel able to value their child's life, whether or not there may be a handicap.

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1 Minerva. *BMJ* 1998;316:240. (17 January.)

Private practice should mirror the NHS

Fee structure for anaesthetists in private practice needs overhaul

EDITOR—I agree with Machin's statement that "It is a point of principle that private practice should mirror the NHS."¹ Equality between consultants has been recognised with identical status and remuneration since the inception of the NHS. The advent of day surgery has resulted in the duties of consultant surgeons and anaesthetists becoming identical with regard to preoperative assessment, intraoperative management, and postoperative care; if anything, the anaesthetist has greater involvement on the day, as the surgeon has already seen the patient in the outpatient clinic. In private practice the surgeon charges a fee for this consultation.

Many anaesthetists believe that the time has come for a radical overhaul of the fee structure in private practice, so that it rewards appropriately the professional skills of anaesthetists rather than reflecting values of a bygone age. Can Machin give an assurance that the BMA's private practice committee supports the basic principle of private practice mirroring the NHS throughout all private practice? Can he explain why this principle of equality should not apply to remuneration for similar commitment in the independent sector?

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1 Beecham L. BMA will seek GMC advice over BUPA scheme. *BMJ* 1998;316:784. (7 March.)

Reply from chairman of BMA's private practice committee

EDITOR—The speech that I made in council concerned the attempts made by BUPA to influence consultant referrals by general practitioners and hospital admissions by consultants through the BUPA consultant "partnership" scheme. I was concerned that patients should be treated in a similar fashion in both the NHS and the private sector.

It would be foolhardy to give a reassurance that the BMA's private practice committee supports the basic principle of private practice mirroring the NHS throughout all private practice, because the two systems are so different that they cannot be compared directly in many areas. Major changes would have to be made to one or both systems to enable us to achieve anything approaching Atkinson's requirement. The NHS is a salaried service with additional benefits such as distinction awards and pensions whereas the private sector works on the basis of a fee for an item of service, with the procedure determining the fee, irrespective of whether it is done on a day case or inpatient basis. Would Atkinson be happy with a salaried private sector? Any threat to employ consultants in private hospitals would be greatest to anaesthetists, who provide for all surgical specialties.

Earnings in the private sector vary, depending enormously on such factors as situation, specialty, and reputation. The expenses of different specialist groups are different—anaesthetists do not usually have consulting rooms or secretaries. The value of various activities is different—consultations give rise to a lower rate than procedures. Surgeons spend over half of their "private" time consulting or visiting patients after operations, and for this activity there is no specific payment. I do not believe that it would ever be possible to achieve equality between specialties but the question of equity between specialties is one that will be addressed by my committee, and as a member Atkinson is well placed to influence the deliberations.

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Only minority of doctors supported idea of state funded health service in 1945

EDITOR—Macpherson has given a valuable account of how the NHS was finally launched 50 years ago after nearly three years of bitter negotiations, but at one point he tries too hard to be tactful about the attitude of most doctors at the time.¹ It would be less than honest not to challenge his statement that "in 1945 most doctors probably supported the principle of a state funded health service." The evidence is all the other way. Throughout the years 1945-8 it was quite clearly a minority, not a majority, of the medical profession that supported this principle.

As so often, it was not so much what was proposed (broadly supported by all political parties and by the *Lancet*) that aroused such an outcry but intense fear of where it might lead. Eight of us, medical students at the time, signed a letter which started, "We are puzzled by the refusal of an overwhelming majority of the medical profession to serve in the National Health Service."² We were not popular.

In this—and in nearly every similar confrontation since—I have been struck by the way in which so many of the most hard working and the most caring doctors (whether in hospital or in general practice) have been among the most politically inflexible. In the early part of the century Lloyd George, who had great difficulty persuading the profession to accept even the very limited financial help that preceded the NHS, found doctors "unruly and unreasonable." Many years later Kenneth Clark, who was equally experienced at negotiating with all sections of the community, said that he had never encountered any group so difficult to talk to as doctors. Why should this be?

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1 Macpherson G. 1948: a turbulent gestation for the NHS. *BMJ* 1998;316:6. (3 January.)

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